

admirable piece of special pleading, based on a skilful assumption of premisses which, to a careless or biased observer, might seem indisputable.

The tendency to variation is spoken of as something very mysterious, of which no adequate account has ever yet been given. Yet the very simple explanation is no bad one, that where two parents are concerned in the production of any offspring, the product in part resembling each of the producers must of necessity also in part differ from each of them. Between the parents themselves, Mr. Herbert Spencer has shown that differences of age and external circumstances would ensure the requisite want of resemblance in the absence of any other cause.

"The rigid test of mathematical calculation" is then applied to the case of mimetic butterflies, with the view of showing that they could not have been produced simply according to the laws of variation, inheritance, and natural selection. In the application of this rigid test the very first step is a perfectly gratuitous assumption, "that it would require, at the very lowest calculation, 1,000 steps to enable the normal *Leptalis* to pass on its protective form." Who is to prove that fifty differences would be insufficient? An interval of a thousand years might be granted for establishing each one of these variations. Suppose even 50,000, instead of only 50 steps to be necessary, it is another gratuitous assumption that "the smallest change in the direction of the *Ithomia*, which we can conceive in any hypothesis to be beneficial to the *Leptalis*, is at the very lowest one-fiftieth of the change required to produce perfect resemblance." How small a difference must decide the choice made by a donkey placed equidistant between two bundles of hay! Certainly, then, a bird on the wing, having to choose amidst myriads of butterflies, may be determined by an almost infinitesimal distinction. Further, though the whole change may be produced by an immense number of small changes, it is not necessary to suppose that all the changes will be equally small. It is merely begging the question to assume that the first change could not possibly be large enough to be of any use. And if it may be of use, the whole mathematical calculation, based on its being useless, breaks down from the beginning. Again, since the *Leptalis* may have spent 1,000,000 years in arriving at its present likeness to the present *Ithomia*, it is impossible to assert that the normal forms of the two butterflies were as wide apart at the beginning of that period as they are at present. The mimicry having once set in, might be retained by parallel variations. This, indeed, cannot fail to be the case, if the protection is to be a lasting one; for when the *Ithomia* varies in outward appearance, unless the *Leptalis* varies in the same direction, the resemblance will be lost. This progressive mimicry would be more valuable than an imitation in which no changes occurred, since the enemies of a mimetic species would in time become aware of a fraud which had no variations at its command, as birds are said now-a-days to pounce without hesitation upon caterpillars which very much resemble twigs. Even "a rough imitation" may be useful in the first instance, and yet when hostile eyes have long been exercised, and have acquired greater and greater sharpness, finally nothing less than *absolute identity* of appearance may be thoroughly effective. Thus the perfecting of the resemblance will be no "mere freak of Nature," nor shall we be "landed in the dilemma that the *last* stages are comparatively useless" in this procedure.

The array of figures brought forward to prove that the *Leptalis* could not have made twenty steps of variation in the direction of the *Ithomia* by chance, would be much to the purpose if any exponent of the theory of Natural Selection had ever argued or supposed that it could. The calculation takes it for granted that the theory is erroneous, instead of proving it to be in error. Upon this assumption, it might have been put far more strongly, only that a stronger way of putting it would have borne on the face of it the suspicion of some inherent fallacy. It begins by supposing that there are "twenty different ways in which a *Leptalis* may vary, only one of these being in the direction ultimately required;" it might quite as truthfully, or even more so, have said a thousand instead of twenty, and then the second step would have given the chance as only one in a million, instead of one in four hundred. But while the theory of Natural Selection speaks of numerous minute useful variations, Mr. Bennett will not allow that combination of terms. Let them be numerous and minute, if you will, he says, but if small they cannot be useful, if useful they cannot be small. He claims to have Mr. Darwin's own word for it, that a large variation would not be permanent, as though Mr. Darwin had said, "living creatures

have come to be what they are by successive useful deviations of structure permanently propagated, but no large deviations are permanent, and no small ones are useful." It is quite obvious that in the use of relative terms, such as great and small, Mr. Darwin neither intended to stultify himself nor has done so. A thing may be large enough to be useful without being large as compared with something twenty times its own size; and a man may be said to have a huge brain in a very small body, although the body in solid content far exceeds the brain. When Mr. Darwin says that "Natural Selection always acts with extreme slowness," he does not imply that its steps must therefore be so numerous as to be too small to confer any advantage. This would be a contradiction in terms. But the steps may be exceedingly small notwithstanding, and also sometimes separated by enormous intervals of time from one another.

In introducing his own explanation of things, Mr. Bennett affirms that "resemblances, and resemblances of the most wonderful and perfect kind" in the vegetable kingdom, "are in no sense mimetic or protective." This may be so, but it can hardly be said to be proved. When he speaks of "man's reason" having "assisted him so to modify his body as to adapt himself to the circumstances with which he is surrounded," and suggests that the instinct of animals may have assisted them also to modify their bodies by slow and gradual degrees to the same purpose, it is difficult to imagine the process intended, and still more difficult to see how "the slow and gradual degrees" will escape the rigid test of mathematical calculation which Mr. Bennett has elsewhere applied; for if the steps are great they ought not to be permanent, and if small they ought not to be useful. A theory which makes it possible for a bee to "modify its proboscis" by instinct, or for a man to treat his nose in the same manner by reason, seems harder of digestion than the Darwinian.

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Torquay, Nov. 12

MR. BENNETT, in his very able paper read before the British Association at Liverpool, and published in NATURE of the 10th November, calls in question the explanation given by the theory of Natural Selection of the various instances of mimicry found in the animal kingdom.

He bases his argument principally on the fact that the alterations in the early stages being useless to the animal would not be preserved, and that these changes must be very slow.

He assumes that to enable the normal *Leptalis* to imitate a species of *Ithomia*, it may be considered to have gone through at least 1,000 stages, and that no change less than one-fiftieth of the whole alteration effected would be of any use to the insect. He gives us no information as to how he arrives at these figures, and we are left with the idea that they are selected principally because they are what are called "round numbers," and are more easily dealt with in the calculation which he gives us.

Now I think that the number of stages which Mr. Bennett considers it necessary for a *Leptalis* to pass through so as to mimic an *Ithomia* is vastly too great: 1,000 stages means at least 1,000 years.

Let us look at the alteration which frequently takes place in the colouring of a butterfly, possibly in one generation, as shown by varieties of which sometimes only solitary specimens are known, figured in Newman's work on English Butterflies. I need only refer your readers to the figures of varieties of *Apatura iris*, *Epinephele janira*, *Limenitis sibylla*, *Melitæa athalia*. Now can it be contended that it required 1,000 of such stages to effect the alteration?

If any of these variations happened to be useful, there seems no reason for supposing that one stage might not make much more than $\frac{1}{10}$ of the alteration, which Mr. Bennett lays down as being the least which would be useful, and which I agree with him in considering much too small. Why might not one stage make one-fourth or one-sixth of the alteration required?

Mr. Darwin quotes a passage in his work on Natural Selection (page 32) from Sir John Sebright with regard to pigeons, in which he says that it takes three years to produce a given feather, but six years to make a head and beak. If the bony structure of an animal so far above a butterfly can be altered in six years, we surely do not require more than that time to effect an alteration in the colour of a butterfly's wing.

Mr. Bennett states that the early stages of the alteration would be useless to the insect; every one, I think, will grant this, when each stage is only one-thousandth of the whole, but not if it be

a much larger quantity. Here again we may observe the instance Mr. Bennett quotes, the mimicry of *Leptalis* to *Ithomia*. *Leptalis* is normally a white insect, and as such, would be more liable to attacks from its persecutors, as shown by Mr. Wallace, while any variation which gave colour to the wing would make the insect less conspicuous, and being useful to it, would be preserved.

That we are quite ignorant of the laws regulating variation is quite true, and that when we do understand them it will throw much light on these questions is undoubted, and that we may probably find in them some additional explanation for many of the facts now accounted for by Natural Selection; and Mr. Bennett does good service in the cause of truth in reminding us of what still has to be done. S. N. CARVALHO, JR.

London, Nov. 17

FOUR years ago I advanced the opinion that Natural Selection is insufficient to explain the "Origin of Species," and that, rather, the origin of the variations of which Natural Selection is said to avail itself must be looked to for this purpose. I may perhaps, therefore, be allowed to say a few words in examination of Mr. Wallace's explanation of this point in last week's NATURE.

One of the objects of Mr. Darwin has been to show that the existence of species as an absolute entity is a mere idea of our minds; that if we could at the same moment look around us in space, and also backwards in time, we should find the organic world connected together as one whole, one great mass of beings extremely closely allied to each other, and distinguishable only by an accumulation of small and perhaps scarcely appreciable differences. A second and closely-connected object has been to show that this great mass of beings has had a common origin from one primeval ancestor (or at most a few ancestors). These two points are the chief ones involved in the "Origin of Species" question, as it is ordinarily understood; and if they be borne in mind, it will be seen that the doctrine of "Natural Selection, or the Survival of the Fittest," deals with only a small portion of the numerous problems involved in this great question. I am sure that Mr. Wallace, after having written as he has done about man, that in his case other influences than this survival of the fittest have been at work, may reasonably allow importance to other powers than Natural Selection in the case of other organic beings.

If Mr. Darwin's book had been entitled "The Influence of Natural Selection on the Formation of Species," some misconceptions might, perhaps, have been avoided. Its present title undoubtedly tends to convey the idea that Natural Selection is *per se* the Origin of Species. I believe Mr. Darwin, however, holds no such idea.

The picture above alluded to, of a complicated mass of beings connected together by innumerable gradations, is so different from what we find existing around us, that one of the first questions suggested by it is, where are the connecting links? This first question has never yet been answered to any extent, or with anything like adequacy. The links produced are but few, and not sufficient to bear the great weight attached to them. For at no period of the geological record do we find any traces of the general and intimate connection of beings with one another that Mr. Darwin's views would lead us to look for. The creatures composing the organic world at any one given moment were, so far as the evidence of geology goes, separated from one another by lines of demarcation of similar value to those existing among animals now.

What is wanted to explain the phenomena of various limited and defined species arising from one common ancestor is, then, first, a law, or group of laws, to throw light on the origin of variation and dispersion; and, second, another law or laws to explain the limitation and separation of the varieties so produced. It is quite out of the question to suppose that the theory of Natural Selection does all this. Those, however, who have studied Mr. Spencer's work will be well aware that his theory of evolution may be applied to deal with the question in this its more extended light. And I believe that those who wish well for the survival of Natural Selection will do well to insist on its only being considered in connection with a more extensive doctrine of evolution. This is where I think Mr. Wallace errs in his advocacy.

I will not here allude to the question of mimicry more than to say, that Mr. Wallace has never answered, but rather avoided, the chief difficulties I have advanced against it; and that his theories on the subject are undoubtedly open to the objection

that he insists on seeing all the phenomena from the point of view of a natural selectionist, and nothing more. As Mr. Wallace has, however, already discovered that Natural Selection, though applicable to man, is not sufficient, unsupplemented, to account for him, we may hope that he will yet see this with regard to the rest of the organic world. D. SHARP

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The Chromosphere

WHILST mapping down, in preparation for the coming eclipse, all the bright lines that have so far been observed and accurately measured in the chromosphere or solar prominences, I was struck with the absence of a faint yellow line, which I have myself several times observed whilst examining the contour of the sun's disc. This line is probably identical with Angström's absorption line 5883.0 (spectre normal du soleil), D" lying almost midway between D' and the line in question. There is no danger of mistaking it for the bright yellow line seen in every solar prominence, and lying near Angström 5865.1, since the two yellow lines were seen on each occasion at the same time on the more refrangible side of D".

I suppose the D", mentioned in a late communication from Dr. Young, to be identical with the bright yellow line, for it is most improbable that he could have failed either to see or to record the bright line whilst mentioning the faint one, since the latter, as far at least as I have observed, is never visible unless in company with the former.

The only observation that I can at all identify with my own is that mentioned in NATURE, December 16, 1869, where Mr. Lockyer, speaking of the absorption line, which corresponds to the orange line of the chromosphere, says that Padre Secchi's bright line is less refrangible.

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From London to Catania

A FEW practical details as to the best way of getting to Sicily, the accommodation to be found there, &c., may be of use to many readers of NATURE who are thinking of going there next month.

We have first the sea passages from London, Southampton, or Liverpool, to Messina or Malta, of which if any be chosen it will probably be that from Southampton to Malta by the P. and O. steamers, which start every Saturday at 2 P.M., and are nine days on the voyage. (Fares 20*l.* and 10*l.*.) From Malta there are steamers twice a week to Messina; they touch at Catania when the weather permits them to enter the small harbour, otherwise they go on to Messina, so that passengers for Catania must in that case avail themselves of the railway.

Few probably will wish to go the whole way by sea, the land route therefore by which the Indian mails are now sent will be taken; viz., over the Brenner Pass. The night mails leave Charing Cross at 8.45 P.M., Cannon Street at 8.50 P.M., Victoria and Ludgate Hill at 8.30 P.M., and arrive in Dover in time for the Calais and Ostend boats; the line from Calais to Brussels may not be practicable, and so the longer passage to Ostend may be preferred; by going straight on one ought to arrive at Cologne at 4 P.M. the next day (if one goes by Calais one has three hours' rest at Brussels). The day service train, first and second-class, leaves all the stations at 7.40 A.M., and one should arrive at Cologne *via* Ostend at 10.55 P.M. (*via* Calais at 4.50 A.M. next day.) From Verviers to Cologne there are only first-class carriages in this train. The fares to Cologne by Ostend are 3*l.* 8*s.* 10*d.* first, and 2*l.* 9*s.* 5*d.* second-class, by Calais they are 3*s.* or 4*s.* more.

Those who like to go from London to Ostend or to Antwerp *direct* can leave St. Katherine's Wharf by steamer on Sunday, Tuesday, or Thursday mornings for Antwerp, or on Wednesday or Saturday mornings for Ostend, and proceed by rail to Brussels, the fares from London to Brussels being 30*s.* first and 22*s.* 3*d.* second class, *via* Antwerp; 26*s.* 8*d.* first and 20*s.* 10*d.* second class, *via* Ostend. The fare from Brussels to Cologne is about 25 francs first and 18 francs second class by the ordinary trains; express about three francs more.

The way then is by Coblenz, Mayence, Darmstadt, and Aschaffenburg to Munich. By leaving Cologne by the 6 A.M. express, one ought to get to Munich at 9.10 P.M. In times when through-tickets are granted the fare by Ostend and Cologne to Munich is 6*l.* 7*s.* 3*d.* from London (first class), and 5*l.* 10*s.*